

June 29, 1951.

Dear Cavalli:

We returned just a few days ago from the Cold Spring Harbor Symposium. I hastened to have a microfilm print made of my draft ms., so that this could be sent you as I had promised- and here it is. I hope you will not be too badly inconvenienced in reading it, but the original was so bulky and I had so few copies that it would have been impractical to send it.

This paper is a rather exhaustive summary of the work we have been doing to date, although a few things are not greatly emphasized. I regret that the figures and many of the tables were not completed in time to include with the prints, but they will appear in the published symposium at any rate.

Our only new project is a minute one: to apply the velvet transfer technique to the direct proof of the recovery of resistant mutants without direct selection in a variety of systems. The indirect selection provides a non-statistical argument that any bacteriologist should be able to understand, and it seems to be working satisfactorily. The approach is to make duplicate impressions of a streaked-out plate of untreated bacteria to plain and to selective medium. The site corresponding to a mutant is used as the inoculum for a fresh plate of plain medium, and so on, until discrete resistant colonies can be picked from bacteria that have never been exposed directly to the selective agent. Already, the duplicate impressions have shown clearly that resistant mutants often occur in clones, expressed as colonies at the same site on a series of replicate selective plates. This is education rather than research, but owing to the recent findings of Eagle, who probably does have evidence of physiological adaptation, with transient inheritance, to drug resistance, there has been a fresh outbreak of Hinshelwoodian confusion over here.

My main work now is on the comparative genetics of different isolates; Mr. Zinder is, of course, pursuing the Salmonella story. DeLamater has been claiming that a mitotic cycle can be perceived amongst the various configurations of chromatin granules; he may be right, but it seems a case of "Rohrschach cytology". Mrs. Lederberg is continuing studies on the transmission of lambda, but I hope we will soon return to the question of the complexity of the Lac<sub>1</sub> locus.

We have some preliminary work, but this fall I hope to have an associate to work full time on the immunogenetics of the various strains.

Do you have much new on Hfr?

Did you see Bailey's paper in Heredity- it may be sound, but rather straining at a flea.

Sincerely,

Joshua Lederberg